SCIENCE:

A WEEKLY RECORD OF SCIENTIFIC PROGRESS.

JOHN MICHELS, Editor.

TERMS:

PER YEAR,	FOUR DOLLARS
6 MONTHS,	TWO "
3 "	ONE "
SINGLE COPIES,	TEN CENTS.

PUBLISHED AT

TRIBUNE BUILDING, NEW YORK.

P. O. Box 3838.

LONDON, ENGLAND, - - - - 150 LEADENHALL ST.

SATURDAY, OCTOBER 29, 1881.

TO OUR ENGLISH READERS.

We have received from Messrs. Deacon & Co., of 150 Leadenhall street, London, England, a standing order for a large supply of "SCIENCE," which will be forwarded weekly. We shall be obliged if our English readers will make this fact known to their friends.

THE WARNER PRIZES.

WE afforded to Professor Swift ample space in our last week's issue, to reply to our strictures on his disposition of Mr. Warner's prize for Comet *b*, 1881. Our readers have now the facts before them and can judge for themselves on the merits of this matter. For ourselves we would say that, realizing the benefits that may accrue from Mr. Warner's gifts, we are not disposed to be too critical in regard to the benefactor nor to the dispenser, and we are far from supposing that either are knowingly walking in the paths of what Professor Swift calls "crookedness." But reading Professor Swift's reply, we cannot interpret it otherwise than as a confirmation of our objections to the course he has taken.

We admitted that, in this instance, under the conditions of the Warner prizes, no claimant could justly claim the prize. We followed by asserting, that as Mr. Warner waived the special conditions and told Mr. Swift to give the $200 to the man who first saw the Comet, it was his duty to have carried out his instructions to the letter.

Professor Swift confirms the position we took on this subject; in his letter he says: "all conceded that no just demands could be made on Mr. Warner" in regard to Comet *b*. Then Mr. Warner said, "inasmuch as the Comet was such a large and brilliant one, and as so many people seemed not to have understood the conditions imposed, *he would offer a special prize of* $200 TO THE ONE WHO I, after an examination of claims, should decide HAD FIRST SEEN IT."

Now comes the *muddle*. Mr. Warner admits that *under his conditions no one can claim the prize;* and therefore offers a special prize for THE ONE WHO FIRST SAW THE COMET. And yet Professor Swift in his letter of explanation says: "*the conditions of the original prize* were, neither in this nor in any other, *to be deviated from;*" and on this account concludes that "not an astronomer in the world would have awarded it."

What can be said or done with men who are so thoroughly and flagrantly inconsistent? Mr. Warner's course throughout appears to have been thoroughly practical; he saw the difficulty in awarding this particular prize, and met it in a most liberal spirit, and had his intentions been carried out, the thanks of the community would have been the unanimous response.

Passing over Professor Swift's apparent misinterpretation of Mr. Warner's instructions, the question may be asked: could "the one who had first seen it" be named? Waiving the claim of the "1000 persons with affidavits" who claimed to have seen the Comet in the United States before its possible appearance, and the 2000 other clod-hoppers and rustics whose claims appeared to have clouded the judgment of Professor Swift, we offer a few simple facts in regard to the first discoverer of Comet *b*, which would have influenced our judgment if called upon to decide on this matter:—

We believe that the first person in the United States who saw the Comet in question, noted its position, and duly reported the fact to Professor Swift was Mr. Edgar L. Larkin, of New Windsor, Ill. If Mr. Warner, however, prefers to award the prize to the *first person* who saw the Comet, irrespective of locality, then we are advised that the following facts bear on the subject:—

Dr. Gould's name was mentioned prominently in connection with its discovery, but according to his own statement, his attention was directed to it by his assistant, Mr. Wilson. But prior to this date it had been observed by Cruls, in Brazil, and also by several English astronomers at Melbourne. It now appears that Mr. John Tebbutt, of Windsor, New South Wales, is credited as the first astronomer to get an observation of this Comet; so that if the prize is to be awarded to the first discoverer, Tebbutt appears to be the man.

The assertion in Professor Swift's letter that Mr. Warner, without consultation with any, pays the prize in certain cases, causes us some surprise, as we thought that his previous experiences hardly warranted him to decide on matters astronomical, and that he delegated the task to others.

In regard to the prize essay, we would advise Mr. Warner to postpone the time of entry until January the 1st next, which will give a reasonable time for some creditable work to be done. We would also propose that the judges be named immediately. Professor Swift says in his letter, "as to who will appoint the judges I am as ignorant as are you." Who does know? Surely Mr. Warner will not propose to decide this matter.

In making these remarks we are far from desiring to disparage the value of such prizes as those offered by Mr. Warner. We understand that Mr. E. E. Barnard, who secured the last prize, is a young man under twenty-five years of age, and a self-taught astronomer. Under very discouraging financial circumstances he provided himself with a good five-inch telescope, with which he has done excellent work. His Warner prize will be turned to good account, as he writes to inform us that the $200 will enable him to purchase a plot of ground on which to build a house for his family; we need not add that an observatory will be a leading feature in Mr. Barnard's new house.

We feel a pleasure in showing the practical good Mr. Warner is doing by providing these scientific prizes, and we trust he may continue them during the following year. Our criticism is of a perfectly friendly character and made with some regret. We have received letters from subscribers confirming our view of the case, which will remain unpublished, as we desire to close the discussion.

ON THE DISCOVERIES OF THE PAST HALF-CENTURY RELATING TO ANIMAL MOTION.

By J. BURDON-SANDERSON, M. D., L.L.D., F.R.S.

[*Concluded from Page* 486.]

The living muscle of a frog is placed in a closed chamber, which is vacuous—*i. e.* contains only aqueous vapor. The chamber is so arranged that the muscle can be made to contract as often as necessary. At the end of a certain period it is found that the chamber now contains carbonic acid gas in quantity corresponding to the number of contractions the muscle has performed. The water which it has also given off cannot of course be estimated. Where do these two products come from? The answer is plain. The muscle has been living all the time, for it has been doing work, and (as we shall see immediately) producing heat. What has it been living on? Evidently on stored material. If so, of what nature? If we look for the answer to the muscle, we shall find that it contains both proteid and sugar-producing material, but which is expended in contraction we are not informed. There is, however, a way out of the difficulty. We have seen that the only chemical products which are given off during contraction are carbonic acid gas and water. It is clear, therefore, that the material on which it feeds must be something which yields, when oxidized, these products, and these only. The materials which are stored in muscle are oxygen and sugar, or something resembling it in chemical composition.

[1] Ludwig's first important research on this subject was published in 1881.

And now we come to the last point I have to bring before you in connection with this part of my subject. I have assumed up to this moment that heat is always produced when a muscle does work. Most people will be ready to admit as evidence of this, the familiar fact that we warm ourselves by exertion. This is in reality no proof at all.

The proof is obtained when, a muscle being set to contract, it is observed that at each contraction it becomes warmer. In such an experiment, if the heat capacity of muscle is known, the weight of the particular muscle, and the increase of temperature, we have the quantity of heat produced.

If you determine these data in respect of a series of contractions, arranging the experiments so that the work done in each contraction is measured, and immediately thereupon reconverted into heat, the result gives you the total product of the oxidation process of heat.

If you repeat the same experiment in such a way that the work done in each contraction is not so reconverted, the result is *less* by the quantity of heat corresponding to the work done. The results of these two experiments have been found by Prof. Fick to cover each other very exactly. I have stated them in a table[1] in which we have the realization as regards a single muscle of the following forecast of Mayer's as regards the whole animal organism. "Convert into heat," he said, "by friction or otherwise, the mechanical product yielded by an animal in a given time, add thereto the heat produced in the body directly during the same period, and you will have the total quantity of heat which corresponds to the chemical processes." We have seen that this is realizable as regards muscle, but it is not even yet within reach of experimental verification as regards the whole animal.

I now proceed abruptly (for the time at our disposal does not admit of our spending it on transitions) to the consideration of the other great question concerning vital motion, namely, the question how the actions of the muscles of an animal are so regulated and coordinated as to determine the combined movements, whether rhythmical or voluntary, of the whole body.

As every one knows who has read the "Lay Sermons," the nature and meaning of these often unintentional but always adapted motions, which constitute so large a part of our bodily activity, were understood by Descartes early in the seventeenth century. Without saying anything as to his direct influence on his contemporaries and successors, there can be no doubt that the appearance of Descartes was coincident with a great epoch—an epoch of great men and great achievements in the acquirement of man's intellectual mastery over nature. When he interpreted the unconscious closing of the eyelids on the approach of external objects, the acts of coughing, sneezing, and the like as mechanical and reflected processes, he neither knew in what part of the nervous system the mechanisms concerned were situated, nor how they acted.[2] It was not until a hundred

[1] RELATION OF PRODUCT AND PROCESS IN MUSCLE.
(Result of one of Fick's Experiments.)

Mechanical product	6670 grammemillimetres.
Its heat value	15.6 milligrammeunits.
Heat produced	30.0 "
Total product reckoned as heat	54.6 "

[2] Descartes' scheme of the central nervous mechanism comprised all the parts which we now regard as essential to "reflex-action." Sensory nerves were represented by threads (filets) which connected all parts of the body to the brain ("Œuvres," par V. Cousin, vol. iv., p. 359); motor nerves by tubes which extended from the brain to the muscles; "motor centres" by "pores" which were arranged on the internal surface of the ventricular cavity of the brain, and guarded the entrances to the motor tubes. This cavity was supposed to be kept constantly charged with "animal spirits" furnished to it from the heart by arteries especially destined for the purpose. Any "incitation" of the surface of the body by an external object which affects the organs of sense does so, according to Descartes, by producing a *motion* at the incited part. This is communicated to the pore by the thread and causes it to open, the consequence of which is that the "animal spirit" contained in the ventricular cavity enters the tube and is conveyed by it to the various muscles with which it is connected, so as to produce the appropriate motions. The whole system,